its claim to the virtues supposed to inhere in it. It has never outgrown its vices, because it has never deigned to beware of them. And so it never improves. It is always an expensive government, because it requires enormous supplies of man-power, even for the simplest *routine* work. It is always pedantic and slow, because it insists on applying the methods appropriate to such work to every problem. It is always unprogressive, because the adoption of any improvement gives uncompensated trouble to the officials who introduce it and upsets official *routine*. It is always stupid, because no subordinate will dare to stop an official superior bent on blundering. It always becomes brutal, as its authoritarian infatuation grows. It becomes also more and more rigid, and its approved mentality approximates more and more to that of senile dementia. It is definitely inferior to all other forms of government in the essential arts of deceiving the people, concocting credible lies and conducting propaganda.

And, worst of all, perhaps, it is unteachable, as recent events have shown. At the end of the Great War—itself probably brought on by the technical incompetence of professional diplomatists—when the three greatest bureaucracies had been completely shattered, after bringing the countries they had dominated to unutterable grief, what did the world behold? The remaining bureaucracies all thought *they* had won the war, and conceived the insane idea that what the people wanted was *more* 'good' (bureaucratic) government, more officials even than in war-time, and more official control and state-interference. So they actually multiplied their staffs, raised their salaries, planted out new branches of old departments, and floated gaily on the flowing tide of inflated estimates, without a thought of finance and the day of reckoning, the evil day which they struggled to postpone with all their might. Thus did skilled bureaucratic government complete the ruin of the war!

Is it remarkable, after this experience, that men should ask for something better than bureaucracy, something more intelligent, far-sighted and adaptable? It may be that such a thing is not to be got, either because the foolish devices of civilization have already produced such mental deterioration in man that the requisite intelligence is not being made, or because the problems of a world-wide trade and industry, throttled and tied at every turn by national tariffs and treaties, are too great, and inherently insoluble for any finite intelligence; but it is at any rate clear that the world's political salvation cannot be secured by reverting to the discredited expedients of a bygone age.

F. C. S. SCHILLER.

Mr. Keynes' treatise on *Probability*.

IN a recent issue of *Nature* Dr. Jeffries gives such an indulgent account of Mr. Keynes' new book on Probability as might be expected from one who has recently interested himself in some logical aspects of the subject, on somewhat similar, albeit sounder and more tolerant lines. To the practical worker in statistics the limitations, and, perhaps one may say, the faults, of Mr. Keynes' book are more apparent.

To the statistician probability appears simply as the ratio which a part bears to the whole of a (usually infinite) population of possibilities. Mr. Keynes adopts a psychological definition. It measures the "degree of rational belief" to which a proposition is entitled in the light of given evidence. Often, as Venn has pointed out, have writers on Probability formally adopted some such psychological definition. But when anything has to be proved about this probability, the definition based upon statistical frequency has always to be used. Mr. Keynes gives a great deal of space to formal proofs: Part II. of his book (71 pages) is practically composed of a symbolical logic in which all the laws of probability are duly "proved." Curiously enough, however, no definition of probability whatever enters into these proofs. Probability is introduced surreptitiously, not in a definition of probability, but in the definitions of addition and multiplication! No proof is given that these definitions are satisfied by the ordinary arithmetical processes, when the probabilities are given numerical values. As a matter of fact, the addition and multiplication theorems of probability, are only known to be true. when probability is used in the ordinary statistical sense. This whole elaborate structure of symbolical logic thus proves nothing whatever about Mr. Keynes' "probability."

Such a logical lacuna would be of little importance, if, when he deals with objective statistical probability, Mr. Keynes' conclusions were generally right, or his criticism generally generous; but the book abounds in unnecessary detraction. Laplace is derided for the political inconstancy of two dedications. Of Quetelet we read, "There is scarcely any permanent, accurate contribution to knowledge, which can be associated with his name." Some conclusions of Pearson are said to depend upon "so foolish a theorem that to entertain it is discreditable." A slight, but instructive illustration of the correlation coefficient, by Bowley, receives the following comment, "by this time the student's mind, unless anchored by a more than ordinary scepticism, will have been well launched into a vague fallacious sea." Yule receives only a mild sneer for advocating the experimental verification of calculated distributions.

The question of taste would be of secondary importance, if this elaborate show of critical exactitude were supported by the announcement of valid and applicable criteria, or even by a clear and thorough acquaintance with the subject. On the first question we may consider the problem (p. 47) of finding the probability that the chord of a circle should be shorter than the side of an inscribed equilateral triangle. By tacitly assuming three different populations of chords, it may easily be shown that in one case $\frac{1}{2}$, in a second case $\frac{2}{3}$, and in a third case $\frac{3}{4}$, are shorter than the standard line. Mr. Keynes, however, never gets down to the specification of the population concerned; by p. 63 he has concluded that the discrepancy arises from the different shapes of the elementary areas in which the chords are supposed to lie, without observing, however, that each of the populations considered might have been approached by assigning the chords to elementary areas of many different shapes.

But a more serious drawback is the apparent lack of acquaintance with the modern developments of Statistical Science. Indeed it might be urged that the very project of writing a Treatise on Probability alone, as if it were still an isolated study, is an anachronism, and shows a lack of acquaintance with this branch of Applied Mathematics, of which probability is one of the elementary ideas, and in which the Theory of Probability finds its sole application.

It is difficult to discover any numerical example which appears to be correct. The reader may gain some amusement in attempting to apportion between author and printer the errors in such atatements as the following (p. 344):

> "Thus "the probability of *a* in certain conditions *c* is ½" is *not* in general equivalent, as has sometimes been supposed, to "It is 500 to 1 that in 90,000 occurrences of *c*, *a* will not occur more than 20,200 times, and 500 to 1 that it will not occur less than 19,800 times." "

The author's intention is evidently to contradict Bernoulli's theorem; it is a nice problem, however, to determine how few arithmetical corrections will suffice to bring the above figures into agreement with the binominal distribution.

Besides mistakes in which the printer may have had a share, there are others which appear to arise from an almost wilful vagueness of the author's ideas (p. 340).

> The probability that the proportion of occurrences will lie between given limits varies with the magnitude of $\sqrt{pq/m}$, and this expression is sometimes used, therefore, to measure the precision of a series. Given the *a priori* probabilities, the precision varies inversely with the square root of the number of instances.

The Precision is thus made to diminish as the number of instances increases!

The one practical example in this section is equally unfortunate. It appears that Czuber, calculating the sex ratio of Austrian births, for the period 1866—77, illustrates Bernoulli's theorum by showing that, assuming that the sex ratio of the population is not changing, the number of female births of a subsequent period may be calculated from the number of male births. The nature of the calculation of the probable error is left obscure by Keynes, and it is difficult to reconstruct it from the data which he quotes. His objection, however, is clear: although Czuber's calculation was justified for the period 1877—94, nevertheless from 1895 to 1905 the Austrian sex ratio differs from its previous value by an amount which would very rarely occur by chance. To the present writer this example well illustrates the application of Bernoulli's theorum, for it has enabled us to detect a real change in the Austrian sex ratio at birth, distinguishing it clearly from such apparent variations as may be due to chance: a change, in fact, in the population, not merely in the sample. To Mr. Keynes it appears to illustrate the supposed dangers of applying to a larger sample conclusions drawn from a smaller one: as if our conclusions would have been in any way different, if the earlier years had yielded the larger number of births! It should be noted that even if Czuber were guilty of making a false prediction, without explicitly stating the hypothesis upon which it was based, the failure of the prediction would not be ascribable to any error in the binominal distribution, which indeed provides the only means of testing its success or failure.

In spite of an immense bibliography, Mr. Keynes does not appear to be familiar with the development of statistics; the fact that the binomial distribution, is not in general symmetrical seems to strike him as a novelty (p. 339):

> "It is easily seen that this want of symmetry is appreciable unless *npq* is large. We ought, therefore, to have laid it down, as a condition of our approximation, not only that *n* must be large, but that *npq* must be large. Unlike most of my criticisms, this is a mathematical rather than a logical point."

Poisson, however, early in the last century, discovered the limiting form of the distribution, when *n* is indefinitely large, and *npq* finite: and so established the famous Poisson Series, which has proved of such service in perfecting the technique of the haemocytometer, and seems to be proving equally valuable to bacteriology in connection with the dilution method of estimating bacterial densities. Of the Poisson Series I can find no mention in Mr. Keynes book, though he gives considerable space to an interesting, though unimportant, paper published in 1895 dealing with one aspect of this assymmetry of the binomial. The binomial has, of course, been fully investigated by Pearson.

In conclusion it may be worth while to elucidate the curious tangle of misunderstandings which begins to appear on page 349. On page 351 we read:

> "The case solved above is the simplest possible. The general problem is as follows: If an event has occurred *x* times in the first *y* trials, its probability at the y+1th is (r+x)/(s+y): determine the *a priori* probability of the events occurring *p* times in *q* trials. If the *a priori* probabilitiy in question is represented by ϕ (p,q), we have:
>
> $$\phi(p,q) = \frac{r+p-1}{s+q-1}\phi(p-1,q-1) + \frac{s+q-1-r-p}{s+q-1}\phi(p,q-1)$$
>
> I know of no solution of this, even approximate."

It may be observed that the conditions of the problem are those given by Laplace's rule of succession, when the experiment is preceded by r—1 occurrences out of s—2 trials, instead of *r* occurrences out of *s* trials. The solution is therefore given by Bayes' theorem when *r* and *s*—*r* are reduced by unity, namely:

$$\phi(p,q) = \frac{(s-1)!}{(r-1)!(s-r-1)!}\ \frac{(r-p-1)!(s+q-1-r-p)!}{(s-q-1)!}\ \frac{q!}{p!(q-p)!}$$

as may be easily verified. The discrepancy between this result and that of Bayes' theorem is due to the fact that Bayes and Laplace rightly (on the assumptions of the problem) take the *mean* of the inverse probability distribution as the probability of the next occurrence, while Mr. Keynes without explanation takes the *mode*.

This misunderstanding of the rule of succession serves to explain the curious remark on p. 377.

> "But refinements of disproof are hardly needed. The principle's conclusion is inconsistent with its premises. We begin with the assumption that the *a priori* probability of an event, about which we have no information and no experience, is unknown, and that all values between O and 1 are equally probable. We end with the conclusion that the *a priori* probability of such an event is $\frac{1}{2}$."

But if, as is assumed, all values for the probability between O and 1 are equally probable, then its probability for a first trial is necessarily $\frac{1}{2}$, for this is the mean of the distribution: in other words, in a great number of "first trials" the event will occur "as often as not." In the present writer's opinion the assumption of such equal distribution is usually illegitimate, but it involves no such inconsistency as Mr. Keynes imagines.

It would be unnecessary to occupy so much space with a criticism of a work which will be chiefly of interest to logicians, were it not that statistics is a practical means of research, attempting in all directions the problems which accumulated data present. Statistical Science offers to the applied mathematician a region of thought which may be described almost as unexplored: it is a science, too, in which the English student enjoys exceptional advantages: and if the views of the last section of Mr. Keynes book were accepted as authoritative by mathematical students in this country, they would be turned away, some in disgust, and most in ignorance, from one of the most promising branches of applied mathematics.

R. A. FISHER.

Susan S. Brierley. *An Introduction to Psychology.* Methuen & Co. London. 1921. Price 5s. nett. Pp. 147.

ALTHOUGH this book is written strictly for beginners, being the substance of lectures before the workers' Educational Association, it contains much close reasoning and is not the arid waste of barren platitude which is often offered to the uninitiated. The author draws attention to the considerable difficulties which our obsession with morals has placed in the path of the psychologist. We tend to look at everything through ethical spectacles and are so intrigued with what ought to be that we can hardly see what is. The modern effort to free ourselves from moralising all phenomena is one sign that a Renaissance is at hand, but the shade of Calvin still haunts us, very often where we least suspect it.

The subject is treated throughout from the biological point of view and the book trenches upon the whole field, but we must confine ourselves to a very few comments. Eugenists will be interested in one of Galton's questionnaires, and will be pleased with the emphasis laid upon the use of the frequency curve and coefficient of correlation. A Binet-Simon test is given in full. In the matter of case studies what the Eugenist is most anxious to know about is the psychology of normal, ordinary people, whereas so much of our information is derived from cases deviating far from the average. This is also true of heredity. The